

DRAFT
MINUTES
Scientific Statistical Committee
September 5-6, 2001

The Scientific Statistical Committee met September 5-6 in Sitka, Alaska. All members were present except Jeff Hartmann, Seth Macinko, Ken Pitcher and Al Tyler.

Rich Marasco, Chair
Keith Criddle
Mark Herrmann
Dan Kimura

Jack Tagart, Vice Chair
Doug Eggers
Sue Hills
Terry Quinn

Steve Berkeley
Steve Hare
George Hunt, Jr.

STELLER SEA LION

The SSC received presentations on four Steller sea lion (SSL) agenda items: the Draft Supplemental Environmental Impact Statement (DSEIS); the Draft Biological Opinion (BiOp4); a report from the RPA Committee (RPAC); and, a report from the Alaska Steller Sea Lion Recovery Team (ASSLRT). Tamra Faris, Dave Witherell, Galen Tromble, Sue Salveson, Lew Queirolo, Ben Muse, Mike Taylor, and Mike Downs provided an overview of the DSEIS. Shane Capron and Doug DeMaster summarized BiOp4 and provided information on changes relative to BiOp3. Larry Cotter and Dave Witherell outlined the RPAC's recommendations regarding sub-options under DSEIS Alternative 4. Earl Krygier provided an overview of the ASSLRT report on the RPAs developed as a response to BiOp3. Among other recommendations, the ASSLRT report recommends reconsideration of the "threatened" status assigned to the eastern stock of Steller sea lions and the "endangered status assigned to the western stock of Steller sea lions. Public testimony was provided by Thorn Smith (North Pacific Longline Association), Dave Fraser, Ed Richardson (At-Sea Processor Association), John Gauvin (Groundfish Forum), Donna Parker (Arctic Storm), and Alan Parks (Alaska Marine Conservation Council).

The DSEIS and BiOp4 are prepared to evaluate revised proposed federal actions to regulate the Alaskan groundfish fisheries. The revisions are designed to mitigate presumptive impacts of the groundfish fishery on the survival and recovery of SSLs. Nevertheless, the true cause(s) of the continued decline and lack of recovery of Steller sea lions remain unknown; as does the impact of the groundfish fishery on Steller sea lion survival. Consequently, the mitigation of presumptive fishery impacts on SSLs are provoked from a precautionary consideration of their potential for adverse effects.

The DSEIS and BiOp4 reflect substantial improvements in analysis and balanced treatment of available information, alternative hypotheses, and explicit statements of assumptions and thus address many of the concerns expressed by the SSC in comments on previous BiOps. However, there are a number of issues that the SSC would like to flag for the attention DSEIS readers and as subjects that could be elaborated or addressed in future studies.

SEIS

Significance Ratings

The DSEIS uses a system of classification for the significance of assumed impacts of the alternative proposed management actions. One of criteria for this system is the proportional change in annual TACs. The SSC recommends that TAC not be used as metric for significance classification. The global

availability of pollock, Pacific cod and Atka mackerel is determined to be adequate to meet the foraging needs of SSLs in BiOp4 (p 147, lines 18-21): *“The effects described above indicate that the fisheries as proposed, are not likely to reduce the abundance of prey within local foraging areas and alter the distribution of groundfish prey in ways that could reasonably be expected to reduce the foraging effectiveness of sea lions, therefore, it would not reduce the likelihood of their survival and successful reproduction nor their likelihood of recovery in the wild.”* The DSEIS rates the impacts of harvest of prey as “conditionally significant negative (adverse)”. This inconsistency needs to be resolved.

Cumulative effects

The goal of this section is to identify and analyze potentially cumulative effects of influences other than the directed federal action that may act additively or synergistically with the directed federal action to cause “major resource trends in the BSAI and GOA ecosystems.” This effort represents the first time that a Cumulative Impacts section has been prepared for an EIS involving a North Pacific groundfish management actions. It also appears that under NEPA guidelines Cumulative Impact sections will be a regular feature of future EIS’s.

The SSC welcomes this effort and finds it to be of considerable potential value. However, the SSC also found this section to be confusing, poorly structured in some areas, considerably lacking in explanations and literature referencing, and full of questionable, unjustified scorings in the dozens of tables (representing thousands of scores). Two general categories of external influences were considered: human controlled and natural events. The climate section is brief and not integrated with the climate section of the BiOp.

A few specific examples of some of the shortcomings include:

- For the impacts on most fish species, three levels, or temporal categories, of climate effects are considered: short term, long term and regime shifts. None of these is well explained. For prohibited species, external natural events are contained in a single category: Climate and Regime Shifts. Marine Benthic habitat has four categories including a Wind induced waves category. Predator Prey and Biological Diversity are back to one category.
- The difference between long term climate variability (LTCV) and regime shifts (RS) is not explained. For every single category, the same score is given for LTCV and RS.
- For none of the categories except Steller sea lions and predator-prey relationships is anything more than a single sentence devoted to explanation of the climate impact ratings. SSLs merit one short paragraph.
- p. 4-427: “Lingering past influences on prey availability from foreign and domestic fisheries, and climate variability and related ecological regime shifts are identified for both the BSAI and GOA Pacific cod stocks.” This statement is not supported by analysis or literature reference.
- There is no need in this impact analysis for 3 or 4 climate categories - a single category would suffice. In the discussion, an assessment of the importance and (if known) nature of a cumulative climate impact existed can be made.
- It would be of interest to determine who/how the scores were made. A climate influence is asserted for all fish species except GOA thornyheads and Pacific halibut. It is asserted that climate effects on halibut are “negligible because halibut are apex feeders.” Halibut don’t begin life as apex feeders and several papers have been published on the effect of climate on halibut recruitment. It is not possible to determine how these statements were arrived at.

Monitoring/Observers

The August 2001 Draft Biological Opinion (section 8) and the SEIS executive summary (ES-20 and ES-21) both contain a list of terms and conditions that are associated with the RPA “reasonable and prudent” monitoring measures. These lists contain terms that have statistical connotations: “statistically robust,” “information should be sufficient to determine appropriate closures”, and “statistically valid”. Although such terms imply a certain statistical precision in estimates, they are general terms that do not have exact meaning. In addition, estimates of statistical precision cannot be obtained unless estimates are made under a proper statistical framework. Many aspects of current analytic practice preclude such estimates; for example the lack of random assignment of observers to the 30% coverage fleet, the use of blend estimates, and the lack of replication information in observer catch estimates.

It should be understood that Alternatives 2-5 would all create greater demands on the Observer Program. This would be mainly through the creation of an increased number of quota categories (i.e., time, area, and species) categories for which catch estimates have to be made. The current number is 27, which will be increased to 46 under the preferred alternative. This causes several problems:

1. A sufficient number of observers must be placed in these additional categories just to allow estimates to be made.
2. Observers will have an increased difficulty concerning the correct categorization of data into species, area (especially), and time categories.
3. Since precision is a function more of absolute sample size rather than percent of catch that is sampled, inevitably, the precision of catch estimates in any particular quota category will be worse than over the current broader categories. This is inevitable without significantly increasing the size of the Observer Program.

Maintaining precision at adequate levels may require a substantial increase in Observer coverage with associated increases in cost to NMFS and commercial fishermen. The DSEIS doesn’t contain an examination of these issues. The SSC believes that this deficiency should be addressed.

Seabirds

There is an underlying assumption that, for Steller sea lions and other top predators including seabirds, more pollock is better for the predator than less pollock. For those predators that eat adult pollock, there is little question that if the local (vs. global) biomass of pollock falls below some unknown threshold, there will be a negative impact on the ability of sea lions to use this prey. However, adult pollock may displace or consume more energy-rich or easily caught prey such as capelin, and particularly age-1 pollock. If it turns out that juvenile sea lions depend on small forage fishes, elevated biomasses of adult pollock near rookeries and haulouts may create formidable competition for critical prey resources. There is a need to examine the implications of such a relationship and how it would influence the outcomes of the various alternate management actions. There is evidence for a negative relationship between the biomass of pollock on the eastern Bering Sea shelf and the productivity of at least one species of piscivorous seabird at the Pribilof Islands.

Other specific comments:

Page 4-218 top: States that effects of fishing will have only an insignificant effect on prey abundance and availability to seabirds. What of the possibility that the removal of adult pollock would enhance prey availability to seabirds?

Page 4-219: Disturbance to seabirds should be a non-issue.

Page 4-222, middle; 4-223 bottom: The statement that less fishing could result in more fish being available to seabirds is almost certainly wrong.

Modeling

4. p. 4-95 Fig. 4.2.1-2 The line describing the control rule under amendment 56 has a discontinuity at relative spawning biomass=0.05. The rule should intersect the axis at 0.05 and be continuous.
5. p. 4-96 Step 3. The solution to this equation is later described as F_{00} , but is never described as such in the text.
6. p. 4-97 top line. Should be for " $a \leq a_{nage}$ "
7. p. 4-97 Step 7. The equation is missing after "For $1 < a < nage$ "
8. p. 4-98 Table alternative 4, middle row. Should be $0.5B_{ref} \leq B_{t,u} < B_{ref}$
9. p. 4-99 4th row from top. "proofed" should be "proved"
7. p. 4-100 Market Constraints (MC) section. 5 lines from bottom of page. It is not clear what "Note that market constraints were not used in this analysis" means. Why is this material presented?
8. P. 4-101 7 lines from top change "heaven" to "100%"
9. P. 4-101 4 lines from bottom "once" should be "ones"
10. General comment on the LP. Many values are constrained both below and above. Question: how does this affect the results? How often do simulations provide solutions on the boundaries?

Consideration of Alternatives and Options

The SEIS essentially rejects Alternatives 3 and 5 in favor of Alternative 4 being designated as the preferred alternative. In viewing the lists of evaluative criteria, the SSC notes that alternatives 3 and 5 appear to have some biological benefits over Alternative 4 but negative economic consequences. It would be useful to have further clarification about how the tradeoffs among these Alternatives were made in leading to Alternative 4 being designated as the Preferred Alternative.

Forage Fish

The term "Forage Fish" is used in different ways. In the section on the prey requirements of Steller sea lions, Pacific Herring and Sand lance are included specifically, whereas in section 3.4, on forage fish, herring are not included. Additionally, age-1 pollock, at present the primary forage fish consumed by seabirds and possibly also important to sea lions, are not discussed explicitly. There needs to be some background information of the importance of both herring and juvenile pollock as forage fishes, and analysis of the potential effects of the various action alternatives on the likely availability of these two species of forage fish to sea lions. It is also possible that these fish should be analyzed in the cumulative effects section.

Draft Biological Opinion (BiOp4):

During 2001, the SSC was tasked by the NPFMC to critically review the November 30, 2000 NMFS Biological Opinion on Alaskan groundfish fisheries. The SSC has had the present BiOp for a very short time and has not had time for as thorough a review of all aspects of the document. Silence from the SSC in these minutes does not necessarily imply agreement or acceptance of statements in the Draft BiOp or DSEIS.

BiOp4 is much more deliberate than BiOp3 in its attempt to describe the standards used to evaluate jeopardy and adverse modification of critical habitat (Sec. 1.7 beginning on p 12). This is a welcome improvement.

The SSC had been critical of the failure of the prior BiOp to discriminate between an objective characterization of the state of knowledge about SSLs and fishery impacts on SSLs and the assumptions or inferences drawn from the objective data. BiOp4 takes care to be more specific regarding the critical assumptions required to reach conclusions that lead to rationalization of proposed management actions. A good illustration of this point is the description of the analysis of the “Forage Ratio Method” used to evaluate modification of critical habitat (p 141, Sec. 5.4.2.1).

Effective experiments to test the efficacy of SSL protective measures

In previous comments the SSC has repeatedly supported adaptive management and effective experimental design to assess the efficacy of SSL protective measures. The SSC has stated repeatedly that learning can only occur with a valid experimental design that includes contrast in the protection treatments. The failure to have done this a decade ago means that we still have learned nothing about the efficacy of the measures. The design must follow solid scientific principles, including testable hypotheses, evaluation of assumptions, and power to detect differences in trend. Although much new research is now being carried out, including the small-scale experiments to test the localized depletion hypothesis for pollock and Atka mackerel, no directed work is planned to link changes in fishery management with SSL status. The SSC regards monitoring the efficacy of the RPA measures essential and recommends development of requisite experimental design to accomplish this task.

Global control rule

The modifications to the global control rule under Alternatives 3 and 4 are shown in Figure 4.2.1-2. Under Alternative 3, fishing mortality is reduced for all biomass levels below $B_{40\%}$ and set to 0 below $B_{20\%}$. Under Alternative 4, fishing mortality is set to 0 for all biomass levels below $B_{20\%}$ and left unchanged at higher biomass levels. The SSC recommends that these modifications, if approved, be done at the TAC level rather than the ABC level. The main reason for this is that if ABC is set to 0 at biomass levels less than $B_{20\%}$, then all fishing will likely cease, if biomass drops to this level, not just directed fishing (presuming the Council continues to constrain TAC at or below ABC). Because the intent of the proposal is to stop only directed fishing, the rule modification should be: TAC = “bycatch only,” when $B \leq B_{20\%}$.

Another consideration is whether a multi species rule is desirable (e.g., stop all directed fishing on pollock when the combined biomass of pollock, cod and Atka mackerel drops below 20% of pristine.) A rationalization for this type of alternative is that if one species is at a high abundance level, then it might not be necessary to lower fishing mortality on the reduced species beyond that provided by the already-conservative status quo policy.

Finally, the SSC is skeptical that these modifications of the control rule will provide any measurable benefits to the SSL population. (1) The current control rule is one of the most conservative used in the world. (2) The likelihood of the pollock, cod, and Atka mackerel population falling to the $B_{20\%}$ level is likely small, as seen in recent stock assessment evaluations of overfishing. (3) If events did lead to a drop in the fish population, it's not clear that the modification would provide for significantly faster rebuilding of fish stocks. (4) It's not clear that $B_{20\%}$ is the level that has anything to do with SSL foraging needs. (5) Changes in the fish populations are most likely to be related to environmental changes affecting recruitment. It would be useful to have a simple analysis of (1) the likelihood of these populations dropping to $B_{20\%}$, and (2) how long it would take to rebuild if reduced to $B_{20\%}$.

Nevertheless, there is a variety of fisheries and ecological literature to suggest that fish populations levels under $B_{20\%}$ are undesirable. Below this level, depensation in the stock-recruitment relationship (reduced survival as biomass decreases) can occur, which can lead to long-term lack of recovery of the population. At the ecosystem level, predators often take only 10 to 20% of prey production, and low prey population levels can limit predators even when considerable prey remains untaken. Thus, there is reason for concern that excessively low local populations of prey would be of concern.

Fortunately, the current control rule and NPFMC's TAC-setting process contains many safeguards to avoid this situation. In particular, the annual review process (analyst, Plan Team, SSC, AP and Council) would likely lead to ABC and TAC downward adjustments in situations where declining recruitment, over exploitation, or other danger signs are apparent. The current control rule leads to the maximum permissible ABC and downward adjustments occurred several times in the most recent stock assessment.

Telemetry Data:

Telemetry data form a cornerstone of the rationale for protection of specific habitats or habitat zones (3, 10, 20 nm protection zones). The interpretation of the data is controversial. The generic model for SSL distribution implies that they are predominately near-shore inhabitants and presumptively, near-shore foragers. Nevertheless, actual foraging distribution remains uncertain. The display of the distribution of telemetry "hits" has been criticized as potentially biased, and BiOp4 presents a hypothetical treatment of the assumed bias. It is important that the BiOp acknowledge that analysis of telemetry data hypothesizes a differential probability of siting based on distance from shore. The assumed bias may or may not be present; continued analysis of existing telemetry data and acquisition of new data will shed light on this question in the future.

BiOp climate section (4.4.1.1)

The climate section in the BiOp (Section 4.4.1.1) is not well integrated with the climate section of the SEIS. If the regime shift is going to be advanced as a primary hypothesis then this section needs to be better focused. There needs to be a more complete summary of the state of knowledge of long-term climate variability. There are many missing and incorrect literature citations.

Some examples of the lack of coordination

1. The Pacific Decadal Oscillation (PDO) is referred to in the SEIS as a mode of long term decadal climate variability but is not mentioned in the BiOp. Further, it is the 20-30 year regime behavior of the PDO that is behind the regime shift hypothesis affecting sea lion population trends.
2. The references Niebauer and Hollowed (1993) and Benson and Trites (2001) are not in the reference list.
3. There is not, in fact, "considerable disagreement" about the effect of oscillations on the carrying capacity of the North pacific (pg. 74). The studies that are cited are not contradictory and present a coherent picture of how the relative fortunes of different species groups have differed.
4. There is a duplicate Merrick et al. 1987 reference in the reference list.
5. More information on the nature of decadal climate variability is contained in a recent special issue of the journal Progress in Oceanography (published in 2000). Several papers in that volume are relevant to this subject.

Research coordination and oversight.

In our June minutes, the SSC “urged [that] more formal mechanism be formulated to ensure sharing and synthesis of data and ideas.” We commend NMFS for appointing Lowell Fritz as coordinator of the new SSL research funded by NMFS. However, this research will be of greatest value if there is an ongoing effort to synthesize findings and share data and ideas. There is a critical need for NMFS to develop a mechanism for assuring that all sea lion research, regardless of funding source, is synthesized and that new research efforts are able to take advantage of and build upon ongoing work. One mechanism for this could be the development of a core group of investigators, not necessarily from NMFS, responsible for integrating research results and facilitating the synthesis and exchange of results.

Future Research Priorities.

In section 3.7.3 of the BiOp (Expectations for information from new research programs), four areas are highlighted. The SSC would like to add X areas as high priority needs.

Telemetry analysis.

In the section on telemetry data, NMFS recognizes that there is a need to account for the central-place foraging behavior of the sea lions, to account for the length of time they are likely to spend beneath the water, and other factors. They do not account for variation in the size of the area available to animals within the distance zones away from colonies. To adjust for some of these issues, NMFS subtracted 90% of the observations between 0 and 2 miles from shore. It would be useful to develop a null model of the expected uniform distribution of observations and then examine the extent to which the observed distribution of at-sea differs from the null model. This could help to identify preferred foraging zones or particular regions of importance to foraging sea lions. In this regard, there is an urgent need to identify those dives or locations used for foraging as opposed to passage or other activities.

Diets/Competition

In assessing diets of Steller sea lions, the method of reporting used to date has emphasized “percent occurrence”. There is an urgent need to develop the ability to determine the size of prey ingested based on the recovery of hard parts in scats and other material and to use these to determine the biomass and energy contributions of various dietary components to sea lion nutrition. The use of percent occurrence as the sole metric of prey use is potentially misleading.

Central to the issue of competition between fisheries and Steller sea lions for prey is the need to demonstrate that the most vulnerable life-stages of sea lions are taking the same species and size classes of prey and in the same regions and depths as the fishery. Since data are still inadequate to determine the size classes of fish used by adult male, adult female, juvenile and newly weaned pups, additional work is essential to resolve these issues. In the presentation of data on diets and foraging locations, it is particularly critical that NMFS specify the source of data on diets by date, location and age of animals, rather than just referring to “sea lions” as a general category. Likewise, when referring to pollock, it is important to note whether the fish taken by the sea lions are adult, juvenile or of unknown age.

In Section 3.1.1.7.3, on page 3-12, there is a statement that “Steller sea lions specialize feeding throughout the water column in the epipelagic (herring), demersal (arrowtooth flounder), and semi-demersal (pollock, Atka mackerel) zones.” This statement seems contradictory, as the types of fish taken and variety of depths at which foraging takes place suggests a very generalized

forager. There also needs to be caution in statements about the use of prey “when they are densely schooled in spawning aggregation nearshore” as there are no data to support the conclusions about the behavior of prey at the time of capture. This statement apparently derives from the Discussion section of Sinclair and Zeppelin, and is not based on direct observation. It is, however, a reasonable conjecture, based on the known biology of the prey species.

SSL Modeling

As noted in our report on BiOp3, a comprehensive population model should be developed to understand the decline in the SSL population and to determine the impacts of additional actions to protect sea lions. Theoretically and statistically valid structural and time series models could be constructed with the available data. Such models could help address the magnitude and significance of population trends, the probability of continued decline or recovery, and the likely significance of factors contributing to the decline or recovery. We recognize that there has been insufficient time to construct these models, however we encourage their development.

RIR/Economic and Social Effects of the Alternatives (Appendices C, D and F):

1. As noted in the June 2001 SSC review of the DPSEIS, “value” is not a synonym for “gross revenue”. Although it appears that the document authors intend “value” to be understood as “gross revenue,” in most instances the use of the imprecise term “value” creates the potential for confusing gross revenues and net revenues, leading the incautious reader to misunderstand the magnitude of net economic benefits.

Our concern in this matter is not a simple complaint over semantics, rather it is a concern over a peculiar use of terminology in Appendices C and D and throughout the DSEIS that is at best confusing and in many instances misleading. While in most cases “value” seems to be intended as a substitute for “gross revenues”, in other instances “value” is used as a synonym for the price paid to harvesters by processors, as a synonym for the net economic benefit accruing to harvesters or processors, and as a synonym for regional economic impacts. The DSEIS should be revised to replace “value” with the appropriate precise terms. In addition, readers of the DSEIS should be cautioned that gross revenue is not a meaningful characterization of the net economic benefits of fishing. Changes in gross revenues may result in similar or opposite changes in net economic benefits. Moreover, readers of the DSEIS should be cautioned that there is no statistical basis for the claimed (see e.g., section 4.12.1.4) significance of differences in the estimated economic impacts of the alternatives.

2. In section 4.10.2.6 (Effect of State of Alaska managed fisheries, state pollock fishers in Prince William Sound), it should be noted that there is no economic basis for the arbitrary assertion that catch changes up to $\pm 20\%$ are “not significant”, those between $\pm 20\%$ and $\pm 50\%$ are “conditionally significant” and that changes greater than $\pm 50\%$ are “significant”. The impact of changes in the allowed catch will differ among individuals and between fisheries. In fisheries where economic rents have been dissipated in the “race for fish”, even small reductions in the allowable harvest may result in substantial economic losses.
3. The DSEIS is over quick to dismiss the impact of the alternatives on subsistence activities. When considering subsistence, it is essential to account for the role that commercial fishing gear and

especially vessels contribute to the pursuit of subsistence fish and game resources. Even if subsistence harvests are unconstrained under the proposed alternatives, restrictions on the commercial fisheries may adversely affect the opportunity to pursue subsistence activities by reducing the cash available to pay for gasoline, etc, by reducing the number of vessels prepared for operation, etc. In effect, commercial and subsistence catches are jointly produced based on shared use of fixed and variable inputs. Although this overlap is particularly pronounced in the case of the small-boat State waters fisheries, it is also present in the CDQ fisheries.

4. The econometric model of groundfish markets provides an interesting initial characterization of supply and demand relationships. However, limitations in the structure of the model and problems with the estimated coefficients suggest that the results of the model should not serve as a basis for judging among policy alternatives. In particular, the statistical procedures employed in the analysis are not robust or sufficiently rigorous and several of the equations are poorly structured. The failure to find a statistically significant inverse relationships between quantity and price in the demand equations provides strong evidence of these statistical and structural problems. The model and results, as they stand in current form, are not useful. This is most likely the result of the lack of time available for development of the model. The modeling work is sufficiently important that it should be continued. Specific concerns and recommendations regarding the model are detailed in an appendix, below.

Appendix to SSC minutes—General and Specific Comments Regarding the Econometric Model of Groundfish Markets

General Comments

1. The model fails to successfully estimate the most important relationship- that between quantity and price in the demand equations. Until these relationships are determined the model should not serve as a basis for estimating the potential impacts of the alternatives.
2. The equations are estimated using seemingly-unrelated-least-squared (SUR), an inappropriate procedure for this type of model. Instrumental variables must be used to replace right-hand-side (RHS) endogenous variables and estimation should be undertaken using three-stage-least squares (3SLS). Additionally, the reported goodness-of-fit statistics are not useful because they are done on an individual equation basis and say nothing about the performance of a system. Appropriate measures of goodness-of-fit (e.g., RMS%E, and Theil U₂ stats) should be calculated for the whole system of equations through dynamic simulation in a program like SAS ETS. Dynamic simulated changes of prices in response to changes in landings should replace the reliance on elasticities. Elasticities, while interesting for examining small effects, are inappropriate when talking about large changes in quantities.
3. More care must be given to properly identifying each behavioral equation. This is especially important when using a systems-based approach. When using a systems-based estimation the effect of including a misidentified equation, with resulting bias coefficients, will filter through the entire system. Great care must be taken to assure that every equation is properly identified, i.e., all relevant variables included. Prices should be in real terms and quantities as per-capita quantities. It is unclear if this was done. If it were done it should be reflected in the presented equations. Failure to do this violates the assumptions of demand equations being homogeneous to the degree zero and the adding up property.

4. When some variables are statistically insignificant, like monthly indicator variables, they should be dropped unless there are strong theoretical reasons to keep them. More work needs to be done on the choice of the functional forms of the equations. While linear forms offer advantages for estimation, they are often poor characterizations to the true relationships; other functional forms should be explored.

Specific Comments

1. Equation 3: U.S. Price of Pollock Fillets—The DSEIS remarks that “*The coefficient on Q_f has the expected sign but is very small, suggesting that the price of pollock fillets is not affected by changes in U.S. consumption. Put another way, U.S. buyers of pollock fillets are relatively insensitive to changes in price (D-21).*” This statement is incorrect. If the coefficient on quantity is small the price flexibility is likely to be high meaning that, if the reverse holds, that consumption will be very sensitive to prices. If this is true this means that if TAC is cut back that consumers will not bid up prices and is why the coefficient on the quantity variable, if estimated correctly, is small. A correct statement is “Put another way, U.S. prices of pollock fillets are relatively insensitive to changes in quantity.”
2. Equation 5. U.S. Imports of Pollock Fillets—This equation is an excellent example of an equation that is not sufficiently identified as a demand equation. Where are variables to represent income, substitute prices etc? The lack of proper identification may be why the relationship between Q and P is not determined. The statement “*Surprisingly, import demand is found to increase with increases in the price of imports. The price elasticity of import demand is 0.79. This is consistent with the results of E3, where the world price was shown to move with the U.S. price. In other words, low U.S production will be associated with high world prices as well as an increase in demand for imports (D-23)*” is faulty reasoning. First, E3 says that the U.S. price is a function of the world price, not vice versa. But even ignoring this, the reasoning is incorrect. The reasoning for this statement seems to be that if U.S. production decreases this causes U.S. prices to rise, import prices then rise, and therefore as imports replace loss U.S. production you find this upward sloping demand curve for U.S. imports. Again, this is a function of the import demand curve not being properly identified. A properly identified demand curve will sort out the confounding effects.
3. Appendix D. p. D-4. “*Numerous past studies have indicated that the demand for groundfish is elastic, so that poor economic conditions in demand centers will cause (proportionally) large decreases in the amount of product demanded.*” If “elastic” refers to the own-price elasticity of demand (as opposed to income elasticity), the sentence makes no sense. The own-price elasticity of demand refers to the isolated relationship between quantity and prices not poor economic conditions occasioned by a recession, etc., the effects of which are removed in the formation of own-price elasticity.